Complexity science and human geography

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Complexity science has attracted considerable attention in a number of disciplines. However, this perspective on scientific understanding remains ill defined. In this paper, ideas and approaches from complexity science are reviewed. It appears that complexity science fundamentally is driven by ontological decisions on the part of the investigator. This is a result of the epistemological approach fundamental to complexity as it is currently studied, which is based on the construction of computer simulation models of reality. This methodology requires that researchers decide what exists and is important enough to represent in a simulation, and also what to leave out. Although this points to serious difficulties with complexity science, it is argued that the approach nevertheless has much to offer human geography. Drawing on complexity science, renewed engagements between physical and human geography, and between both and geographical information science seem possible, based on clearly shared concerns with the representation of geographical phenomena. In conclusion, it is suggested that seeing models as a source of geographical narratives may be a useful way to promote constructive engagement between different perspectives in the discipline.

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> It is, moreover, evident from what has been said that it is not the function of the poet to relate what has happened, but what may happen – what is possible according to the law of probability or necessity [. . .] It clearly follows that the poet or 'maker' should be the maker of plots rather than of verses, since he is a poet because he imitates, and what he imitates are actions. (Aristotle, Poetics, IX)[1]

Introduction

Recent exchanges in this journal have created a space for critical engagement between various strands in geography. Surprisingly, the impetus has been reflections on simulation modelling.[2] Pondering the implications of a computer model of geomorphological processes, Raper and Livingstone (1995) consider relationships between space and time, and between process and object based conceptualizations of physical geography. This article prompted reflections on 'Space-time, "science" and the relationship between physical geography and human geography' from Doreen Massey (1999), who closes with an open-ended 'Can we talk?' (1999, 274). Raper and Livingstone (2001), together with Lane (2001) respond to her invitation, and Massey (2001) offers further comments. While this hardly constitutes a groundswell, constructive exchanges between such diverse researchers are sufficiently rare to merit comment. This article is a response to Massey's invitation, and an attempt to widen the terms of reference of the discussion.[3] Specifically, I examine *complexity science*, within which much computer model-based research inside and outside geography is now conducted. Although the discussion is limited to human geography (broadly defined), my comments about complexity, modelling and simulation, and about geography's potential contribution, are relevant to a broader audience.

The plan of the article is as follows: first I consider the ill-defined notion of 'complexity science'. Building on this discussion, I suggest that many concepts in complexity science resonate strongly

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with longstanding ideas in geography. This prefaces a discussion of the philosophical stance on reality and knowledge implicit in complexity science, which raises ontological and epistemological issues, particularly regarding the use of computational simulation models in complexity research. These considerations lead to a discussion of how research in the complexity *modus operandi* can be useful, which brings us back to the potential for intra-disciplinary discussion provided by the complexity venture. In concluding remarks I suggest that the changes that have occurred in computer simulation modelling, partly arising from, and partly contributing to, complexity science, have increased the relevance of model-based approaches to understanding in geography more widely.

The complexity of 'complexity'

Complexity is hard to pin down. Like its (still important) spiritual ancestors, *catastrophe* (Wilson 1981) and *chaos* (Gleick 1987), the word itself carries enough baggage from its everyday use to cause problems for any definition. Thrift (1999) demonstrates that complexity has currency in many places, contributing further to confusion over its meaning. Business theorists evangelize complexity as the latest management paradigm in the commercial world (see, for example, Meyer 1998).[4] Complexity is also part of a diffuse set of beliefs that science is reductionist, and that a holistic approach to understanding the world is more appropriate (see, for example, Capra 1996; Kauffman 1995). Publishers have found this intersection of audiences irresistible. The resulting popular science literature ranges from the staid (Holland 1998; Coveney and Highfield 1995), through the excitable and journalistic (Levy 1992; Waldrop 1992), to the hyperbolic, even quasi-religious (Wolfram 2002; Kauffman 1995). The upshot is a whirlwind of conflicting definitions, considerable hype, and more than a little scepticism (see Horgan 1995).

Three types of complexity[5]

Academic writing on the subject is scarcely less confusing. Steven Manson's (2001) review classifies a diverse literature into three categories of research where the term is used: algorithmic complexity, deterministic complexity and aggregate complexity. Rather than reinvent the wheel, these are set out below, before considering aggregate complexity in more detail, because it is the most relevant in geography.

Algorithmic complexity refers to measurement of the difficulty of computational problems. Almost equivalent are attempts to quantify the informational content of data – 'almost equivalent' because the most widely cited measure of information content (Chaitin 1992) relies on measuring the length of the shortest computer program that can reproduce the data. While this may be relevant to the assessment of map patterns, algorithmic complexity has no obvious application to geography more widely.

For Manson (2001, 407–9) *deterministic complexity* refers to the unpredictable dynamic behaviour of relatively simple deterministic systems. Unpredictability is framed as sensitive dependence of outcomes on initial conditions. Also termed *chaos theory*, this perspective is summarized in the poetic notion that the flap of a butterfly's wings in Pennsylvania might ultimately cause a thunderstorm in London. Other evocative terms – strange attractors, fractals – bear witness to an intellectual pedigree as intertwined with the contemporary zeitgeist as complexity (see Rushkoff 1994, for example). However, in spite of an extensive literature in geography (see, for example, Antrop 1998; Batty and Longley 1994; Batty *et al.* 1989; Dendrinos 1992; Goodchild and Mark 1987; Lam and De Cola 1993; Longley *et al.* 1991; Malanson *et al.* 1990; White and Engelen 1994), it has proved difficult to effectively import insights from chaos theory. Jonathan Phillips goes to the heart of the matter, when he asks, '[o]f what relevance is this to the *empirical* earth [. . .] where conditions are not merely unknown, but manifestly unknowable?' (Phillips 1999b, x, emphasis in original).

In fact, chaos is both bane and boon. One conclusion from deterministic uncertainty is the *futility* of prediction, since a comprehensive understanding of mechanisms is useless without omniscience about the empirical world, effectively forestalling attempts at predictive science. As Phillips further argues, 'sensitivity to initial conditions is better understood as *independence* of initial conditions' (1999a, 18, emphasis in original). On the other hand, deterministic uncertainty enables enquiry into intractable systems whose behaviour appears random, since such behaviour may be amenable to understanding on closer examination. As Phillips suggests, this hopeful conclusion can only be justified by bringing abstract concepts from complexity science down to earth (literally in this case), and connecting them to careful observation of processes and mechanisms in the field.

The study of phenomena characterized by interactions among many distinct components is labelled *aggregate complexity* (Manson 2001, 409). This view of complexity is influential, and likely to have a lasting impact on scientific practice. Studying the behaviour of collections of entities focuses attention on *relationships* between entities. According to Cilliers,

> [i]n a *complex* system [. . .] the interaction among the constituents of the system, and the interaction between the system and its environment, are of such a nature that the system cannot be fully understood simply by analysing its components. (Cilliers 1998, viii, emphasis in original)

Implicit is the idea that each entity has different relations to others and, therefore, that *where* an entity is in the system has significance for the unfolding behaviour of entities individually, and of the system collectively. The point is to arrive at understanding by reduction *and reassembly* of a system of aggregate complexity. The critical break with previous reductionist science is the attempt at reassembly, although a reductionist first step appears unavoidable.

While Manson's typology of complexity usefully clarifies the multiple meanings of an over-used term, it is noteworthy that his categories refer to aspects of phenomena that are not mutually exclusive, nor is this the only possible classification (Reitsma 2003; Manson 2003). Algorithmic complexity refers to measuring the level of 'difficulty' associated with some system state. Deterministic complexity refers to dynamic behaviours, which a system may exhibit regardless of its degrees of freedom (its internal structure) or external forcings (its environment), although canonical examples (May 1976; Lorenz 1963) are simple with only a few components. Neither algorithmic nor deterministic complexity is a necessary property of systems that are complex in the aggregate sense. Further, while neither algorithmic nor deterministic complexity implies much about the internal organization of things, aggregate complexity makes strong claims about the structure of the phenomena to which it is applied.

Aggregate complexity is of particular interest to geographers because it implies that the local spatial configuration of interactions affects outcomes at the whole system level. How local interactions 'scale up' to effects at larger scales is a familiar concern to geographers. Nigel Thrift has commented that complexity 'is a body of theory that is preternaturally spatial', and '[w]hereas previous bodies of scientific theory were chiefly concerned with temporal progression, complexity theory is equally concerned with space' (Thrift 1999, 32).

A relational view of space is intended here, because the focus on space centres on how relationships between entities are differently structured across a system.[6] In some cases structuring is more or less purely spatial – transport systems, for example – depending on the temporal scale at which events are viewed. More often, and again depending on scale, interaction structures are configured and reconfigured by spatial effects, but are not spatial *per se*. Thus, the economic relationships in a regional economy are spatially structured, but there is no simple mapping from geographic location to place in the interaction structure. Rather, place is a complex web of social, economic, political and other relations, which are themselves spatially structured and configured over time. These are themes that have become familiar in the human geography literature (Gregory and Urry 1985).

It is noteworthy that even this schematic description of how geographic space configures and reconfigures systems over time is largely *absent* from accounts that explicitly draw on complexity science. This partly reflects the disciplinary biases of 'early adopters' of the approach. In economics, for example, the self-proclaimed 'new economic geography' (Krugman 1991 1994 1999) has attracted a great deal of attention. Whatever the merits of this work (see Martin 1999; Dymski 1996; Isserman 1996; Berry 1994), the pertinent issue is that the representation of space (see also Fujita *et al.* 1999; Arthur 1988) is highly simplified.[7] Furthermore, 'space' enters as a fixed structuring framework for interactions between economic entities (cities and regions). This contrasts with economic geography, which while it only occasionally draws on ideas from complexity theory, sets them within a broader social, economic and political account (see Storper 1997).

In spite of the schematic handling of space, the claim of an affinity between geography and complexity science is warranted, and implies that sustained engagement with complexity theory by human geographers is overdue. In fact, alongside an 'architecture of complexity' (Simon 1962), a case can reasonably be made for a geography (or geographies) of complexity.

Complex dynamics: new names for old ideas

One drawback of focusing on aggregate complexity is the emphasis it places on structural properties of systems. This leads to a misleading static view, because it is the dynamic behaviour of systems of aggregate complexity that really sparks interest among researchers. Complexity science has spawned a rich lexicon of metaphors describing system dynamics, among them *self-organization*, *emergence*, *path-dependence* and *positive feedback*. Space does not permit a comprehensive glossary, but some description of these terms and their consonance with geography is necessary. Broadly, two characteristics of complex dynamics are frequently noted: *self-organization* and *path dependence*. Interpretations are as diverse as the disciplinary settings in which these concepts have been identified, but key features can be described, along with the potential insights provided by viewing them from a geographic perspective.

Self-organization and emergence

Complex systems organize themselves – they *self-organize* – without higher-level direction, into *emergent* phenomena. Self-organization is so central that it has been used to identify the phenomena with which complexity science is concerned. This definition proposes a spectrum of behaviour ranging from ordered and highly structured to disordered or apparently random. The greater part of this spectrum is occupied by complex phenomena that are neither completely ordered nor completely random, but which exhibit non-random structure, combined with sufficient unpredictability for novelty itself to be a persistent feature. Examples abound in the natural and social sciences. Stream networks and coastal formations – the latter providing the setting for Raper and Livingstone's (1995) model – as well as regional economies, language and social systems are characterized by evident structure combined with a propensity for unpredictable and (viewed at certain scales) apparently random change. Richards (2002) contends that self-organization is the key notion from complexity theory for physical geography, and others concur (see Malanson 1999; Phillips 1999a 1999b).

Regarding self-organization as a tendency for complex systems to evolve towards ordered states sets up a tension with deterministic uncertainty. Self-organization implies that many initial conditions of a system *converge* to broadly similar outcomes, while chaos suggests that sensitive dependence on initial conditions leads to *divergent* outcomes. Phillips (1999a) claims that the problem is partly semantic, and partly related to the scale of analysis. Noting *eleven* distinct concepts of self-organization in the physical geography literature, he wryly comments, 'Self-organization clearly means different things to different people' (Phillips 1999a, 481), and goes on to suggest that the apparent contradiction between convergence and divergence is resolved when we consider the geographical scale of analysis. Processes may be convergent in terms of aggregate statistical regularities describing system-level outcomes, while simultaneously being widely divergent in terms of disaggregate outcomes at particular locations. Thus dynamic processes may produce increasing landscape differentiation over time (divergence) at the same time that aggregate regularities are maintained at larger scales (convergence). Parallels with theories of uneven development (Smith 1990) are clear, and scale is also an important determinant of the interpretation of outcomes in that context (Smith 1982).

Related to self-organization is the notion that global phenomena *emerge* unbidden from interactions among lower-level entities. This idea is also laden with tension. By reference to emergence, complexity researchers lay claim to holistic understanding, while retaining a reductionist programme, since the basic entities from which aggregate phenomena emerge are a baseline for analysis in any particular instance. This tension is central to the ontology of complexity science and is examined in more detail below. For now, I note that release of the tension may lie in understanding the effect of spatio-temporal scale on the representation of complex phenomena.

Path dependence, positive feedback and lock-in

Path dependence is summarized aphoristically in the phrase, 'history matters'. This is by contrast with the prevalence of equilibrium concepts in many earlier approaches. In particular, in mathematical economics, equilibrium is a stable, hence static and a-historical state, towards which a system evolves, and to which feedback effects inexorably return the system should it stray far. Path dependence holds that a system's trajectory is a function of past states, not just the current state. Furthermore, *positive feedback* effects are as likely to reinforce current trends as negative feedback effects are to counteract them – indeed, in some circumstances negative feedback can also be destabilizing.

One manifestation of path dependence and positive feedback is *lock-in*. The canonical example is the QWERTY keyboard, which David (1985 2001) argues is a case where early adoption of a standard typewriter keyboard locked the market into one design. The mechanism is the market advantage that accrued to the QWERTY design as a result of positive feedback. As more typists were trained on QWERTY, it became more economically advantageous to stick with it regardless of its inefficiency compared to competing designs. Liebowitz and Margolis (1990 1995) argue that this case of lock-in is not compelling because it relies on suspect claims for the superiority of the alternative Dvorak keyboard. Their more thorough account shows that QWERTY's inferiority is unproven. The alleged superiority of the Dvorak keyboard is critical to David's case because there is no economic 'paradox' of market inefficiency to explain unless a demonstrably superior design existed that nevertheless failed to catch on.

This debate is somewhat artificial, with an equilibrium, optimal keyboard design hovering mirage-like over the argument. It is departure from this idealized equilibrium that David explains using lock-in, and which Liebowitz and Margolis deny.[8] While lock-in remains controversial in economics, it is hardly a novel concept in human or economic geography, where regional difference and economic advantage are subject to the effect. Given that a path is a succession of places through time, the concept of path dependence implies that history *and geography* matter, but this is not emphasized in mainstream complexity science accounts.[9] This suggests, once again, that geographers are well placed to contribute to these broader debates.

Complexity metaphors: tools to think with

Opinions are divided on the importance of self-organization, emergence, positive feedback, path-dependence and lock-in. Even in restricted domains, ambiguity attaches to the concepts. Their real usefulness is that they promote new ways of understanding problems. In a discipline such as economics, where a single paradigmatic perspective – equilibrium – has dominated thinking for so long, this can be heady stuff.[10] Thus, even as the ghost of equilibrium haunts the lock-in debate, this new metaphor succeeds in promoting new thinking.

Attempts to apply metaphors often struggle with confusion between description and explanation. For example, one of the understandings of self-organization identified by Phillips is 'Systematic rank-size distributions' (Phillips 1999a, 468, Table 1). Rank-size distributions are pervasive in the complexity literature. Most familiar to human geographers is the rank-size distribution of cities (see Zipf 1949), which may be considered self-organized, since nobody planned a distribution of city sizes conforming to a neat mathematical 'law'. The consensus conclusion that approximate rank-size distributions are generated by any process where the rate of growth of entities is dependent on the current size of those entities (so that large cities grow more quickly than small ones) begs the question of just how much labelling the phenomenon 'self-organizing' or 'emergent' adds to our understanding.

The uncertain meaning of these concepts echoes ambiguities in complexity itself. Confronted by a multiplicity of metaphors, students of the less mathematical sciences can allow themselves a wry grin at the belated recognition of effects evident to even the most casual observer of the real world. Nigel Thrift remarks that, '[a] cynic might argue that it is because as these metaphors have travelled, so they have become almost completely meaningless' (Thrift 1999, 39). Nevertheless, he argues that shared metaphors are useful, enabling us to see connections between diverse fields. A case can readily be made that these metaphors are in a language that geographers already understand, as I have indicated. This is why it is important to consider complexity, geography's place in complexity and the implications of these new approaches to science for human geography.

Philosophical twists and turns

It is apparent that complexity means different things to researchers. One common thread discernible in the practice of complexity science is the use of computational models. While the prevalence of computational modelling varies across disciplines, the consensus is striking and deserves scrutiny, because shared scientific practices not only change the tools that are used, but also entail ontological and epistemological commitments, some of which are considered below.

Ontology 1: aggregate complexity, emergence and scale

It is a working assumption of complexity science that the world consists of large numbers of interacting entities. For any discipline the interactions of some set of basic entities constitute the subject matter. Chemists study interactions among molecules and atoms, but not the composition of atoms – the domain of physics – or the properties of more complex structures composed of atoms and molecules – which are concerns for molecular biologists and materials scientists. As Anderson notes,

> [t]he behavior of large and complex aggregates of elementary particles, it turns out, is not to be understood in terms of a simple extrapolation of the properties of a few particles. Instead at each level of complexity entirely new properties appear. (Anderson 1972, 393)

This perspective can be developed into a disciplinary hierarchy. Whether we see moves from one discipline to another as instances of 'symmetry breaking' (as does Anderson), or as instances of the truism that the 'whole is more than the sum of the parts', it is clear that 'something happens' when we move from interactions among a few entities to interactions among many. Emergence is therefore critical in many accounts of complexity science.

Of particular relevance to human geography is the consonance between this account and Bhaskar's critical realism with its argument for the emergent properties of social structures (Bhaskar 1998, 37–44; see also Sayer 1992, 117–21). This connection is made explicit by Harvey and Reed (1996, see also Byrne 1998) and has important implications for computational modelling. I want to focus on two issues. First is the tendency to reductionism in complexity applied to the social sciences, notwithstanding a nod to holism, via emergence. A second and related issue, because scale and space are implicated in any consideration of emergence, is the necessity of avoiding simplistic mappings from emergent levels to spatial scales, even if this sometimes provides an organizing framework.

In spite of the clear implication of any theory of emergence that 'entirely new properties' exist at the social level, which are not explicable simply in terms of the behaviour of individuals, there is resistance to this idea in accounts of social systems in complexity science.

This is clear in the literature on multi-agent social simulation (for an introduction, see Gilbert and Troitzsch 1999; for recent examples, see Berry *et al.* 2002). In a multi-agent social simulation, individuals are represented as autonomous software agents with individual characteristics, and the aggregate behaviour of a collection of agents represents society. Proponents suggest that research using multi-agent simulations is a new kind of social science 'from the bottom up' (Epstein and Axtell 1996). I have argued elsewhere that agent-based approaches can be useful, but often make no attempt to consider social phenomena explicitly (O'Sullivan and Haklay 2000). This issue is inherently a spatial one, since 'the spatio-temporal boundaries of any model require the explicit inclusion and modelling of social structures' (2000, 1416). At any particular space-time scale, some social, economic, cultural and other aspects may usefully be regarded as more or less fixed constraints. This is true *even if* the researcher believes that such structural effects are 'ultimately' reducible to the actions of individuals. Further, assumptions about the structural constraints remain hidden unless they are explicitly modelled. This argument acknowledges that the social and the spatial are always and everywhere closely interrelated. While some approaches to multi-agent simulation are beginning to broach the topic of representing the social explicitly (Carley 2002), an individualist bias remains (Sawyer 2003).

It is important in arguing for the importance of the spatial frame and of scale to avoid over-reliance on schematic ideas of a hierarchically nested world composed of interacting basic elements at one scale forming building blocks at the next scale 'up'. Cilliers (2001) identifies a number of issues in this regard. First, a boundary is not merely the 'edge' of a thing separating it from everything else. Equally, it constitutes the thing, and connects it to everything else. Second, there is a strong tendency to think of things as spatially contiguous. Cilliers reminds us that intricate non-contiguous spatial structures are common, that boundaries are often inter-penetrating, and that it may be difficult to distinguish the 'inside' and 'outside' in many cases. Third, following directly from this, hierarchies need not be neatly nested structures building into progressively larger entities at successively larger scales. These observations are particularly relevant in geography where tempting scale-based hierarchies of the neighbourhood–region–nation–supranational bloc type present themselves. While such schemes are relevant in some contexts as organizing frameworks, they decidedly do not reflect the untidy real-world geographies of multinational corporations, nation states, stateless nations, terrorist networks, the Internet, protest campaigns, pressure groups, non-governmental organizations and the rest. These are troublesome issues for many engaged in complexity science, because they disrupt attempts to develop formal schemes for representing and exploring complex systems (see, for example, O'Sullivan 2001).

Epistemology 1: 'experimenting on theories'

A widely adopted strategy for understanding systems *explicitly as complex systems* is the development of models that represent them. Such models are almost invariably computational, enabling researchers to manipulate parameters to see how they affect dynamic behaviour. This approach has been dubbed 'experimenting on theories' (Dowling 1999), and assumes that computational models may be used as controlled experiments in a programme of research aimed at uncovering laws governing the behaviour of complex phenomena.

The viability of this research programme depends on the adequacy of computational models as representations of reality. Two factors favour contemporary complexity science over earlier formal mathematical approaches to modelling the world. First, the representational expressiveness of computational models is considerable. Formal algebraic representations of reality are hindered by a pragmatic preference for mathematically tractable functional forms. Unfortunately, mathematically convenient forms – the infinite isotropic plain of central place theory, for example – are usually poor representations of social reality. By contrast, computational models admit a broader range of representational styles, and while many models remain strongly mathematical, this is not a serious constraint. Second, earlier versions of systems theory lean heavily on assumptions about the form of solutions, so that a concept such as equilibrium, a mathematical convenience with little empirical basis, was often elevated to a central role. Although, complexity science brings its own assumptions about the behaviour of models to bear, these are sufficiently loosely woven – as we have seen, they are distinctly ambiguous, baggy even – not to act as a straitjacket.

It is useful to consider criticism of earlier quantitative modelling in geography, and compare how contemporary computational models stand up to similar scrutiny. Andrew Sayer (1976) is devastating in his critique of urban models rooted in mathematical formalism, static equilibrium and urban economics. He calls for a refocusing of research away from technical aspects of theoretically inadequate models, and onto the political-economic processes driving urban change. He also advocates more 'structuralist' models of cities, while acknowledging that

> A structuralist approach may impel us to include far more in our models than we can handle, so that we may start from trying to understand some small subsystem, and in our search for a system closure [. . .] end up modelling the entire urban system. (Sayer 1976, 249)

There is a clear echo here of the preceding discussion of emergence and hierarchy in complex systems.[11] In fact, many urban simulation models grounded in complexity science at last have the potential to address this quarter-century-old critique. Much current work, consciously or (mostly) not, addresses at least some of Sayer's criticisms. Even where this is not the case, no technical obstacles prevent the approach he recommends, wherein households, employers, developers, financiers, local and regional government and other players on the urban stage might be represented together with their complex interactions, motivations and resulting actions. It is important to temper such enthusiasm: while current models are superior to many earlier purely mathematical exercises because of a richer representational connection to their real-world referents, strong biases in favour of economics remain, with all that entails, and rich seams of other theoretical perspectives remain unexploited (Warf 1995).

Sayer's critique also points to a serious problem for the prospect of learning about the world from models. A claim to be doing experimental science using models as 'computational laboratories' is all very well, but it must be acknowledged that the experimental subjects are models, not the world itself. Understanding the representational connections between the world and a model is important, and some detailed technical issues must be considered here, before returning to the epistemological difficulties raised by 'doing science' with computational models.

Ontology 2: object orientation and representation in complex models

It is difficult to generalize about computational models, because of the wide range of possible representational approaches. Even so, it is instructive to consider *object-orientation*, which has dominated software development throughout the recent expansion and rapid diffusion of computational modelling. This also returns us to Raper and Livingstone's (1995) OOgeomorph model, which was developed using object-oriented methods. Object-orientation (OO) is ancient history by computer science standards, with the first object-oriented programming language, Simula-67, developed in the 1960s (Dahl and Nygaard 1966). However, the approach spent a long time in academia, only becoming mainstream in software development during the 1990s (Booch 1991), following the popularization of OO programming languages such as Objective-C and C++ (in the mid to late 1980s), and Java (in the mid 1990s). OO remains obscure to the uninitiated, who continue to think of programming, if they think of it at all, in terms of the input of data for processing, leading to the generation of output.

In developing an OO system (including a geographical model), the programmer defines *classes* of object types. A class definition enables a computer running the program to instantiate individual *objects* of that class, which are used to solve a problem or simulate a geographic phenomenon. A class definition states what are the *properties* of objects of that class, and what are the *methods* of objects of that class. An object's properties define its current state, while its methods enable other objects to interact with it. A key aspect of OO is that while the properties of objects are usually 'private' and internal to the objects, its methods define a public *interface* through which other objects may interact with it.

It may help to consider the most familiar example of OO: the ubiquitous graphical user interface (GUI) for controlling computer programs. Although they *can* be programmed without OO techniques, in practice, GUI development is heavily dependent on the idea of the computer screen as a collection of objects – windows, buttons, scroll-bars and so on. Each class of object has a well-defined interface to the world, in terms of the messages (or method calls) to which it responds, and how it responds to those messages. Thus, an on-screen button receives a 'click' message from the user via the mouse (itself modelled as an object), the button is seen to depress and something happens. Buttons are simple examples of the advantages of the approach. Other object abstractions allow us to 'drag and drop' icons representing documents, images, webpages or other 'real' things, thus moving the (virtual) object they represent to a new place in the computer's filing system. A major aim of object-oriented programming and design, at which it succeeds almost indecently well, is to make the development and manipulation of complex programs possible. This is achieved by enabling programmers to think in more concrete terms about problems, and so to develop software objects to represent entities in the problem domain.

The relationship between these concepts and an ontological commitment to collections of interacting simple entities is striking and hardly coincidental. The concern in complexity science to define boundaries is exactly analogous with the importance in OO design of identifying what are the object classes in the problem domain, and what are the interfaces to those objects. Object-oriented programming lends itself to the development of computational simulation models of geographic phenomena. Instead of a set of mathematical equations, a geographical model is a collection of software representations of the geographic entities of interest as objects, and interactions between entities are represented by method calls between objects, which cause changes in the internal state of either or both objects involved. In object-oriented programming, it is often hard to tell how a program works, because many tasks are achieved by interactions among objects, so that examining the coding of individual classes in isolation is not very illuminating. Transferred to the domain of object-oriented simulation models, the same mystery attaches to the emergence of aggregate model behaviour, as has been noted.

In short, the representational approach in OO models is one where objects are substantially constituted by the interactions they can participate in. In a model representing an organization, roles might be defined, together with the interactions between roles in the form of methods for handling interactions. The roles are meaningless in the absence of interactions between them. There is again a striking parallel here with the distinction made in realist social science between necessary and contingent relations between social roles. A landlord is defined by her relationship to tenants, and vice-versa, independent of any particular tenants that might be playing that role at a particular time. In OO models, necessary relations are set out in the class definitions, while contingent relations play out as a model runs, and particular instances of landlords and tenants interact.

Two important lessons can be drawn from this brief sketch of object orientation. First, the conclusion is unavoidable that available computational tools have played a major role in the development of concepts and theory in complexity science. This sounds like a criticism, and of course it *is* a criticism of practitioners who fail to reflect on how the tools they use pre-dispose them to seeing the world in particular ways. However, all disciplines evolve in tandem with their tools, and complexity science is no different. Second, more positively, an object-oriented approach forces researchers to seriously confront the nature of the entities they intend to represent in a model. This is the underlying message of Raper and Livingstone's (1995) paper, where the discipline of OO design forces detailed consideration of the nature of space-time and geomorphological phenomena. Any exercise in geographical modelling must sooner or later confront such difficult questions. Taken seriously, this must be considered an important advantage of the computational approach over more schematic modelling methods. Where before the move from reality to formal representation was often under-examined, now representation is central, and must be taken seriously.

Epistemology 2: learning from models

The fact remains that the subject of study in complexity science is often not the world itself, but computational models representing the world. How models may be used to learn about the world – if at all – is a critical epistemological question. This question is important for the lone academic researcher who wants to develop a better understanding of the world using a model, and to avoid the nagging feeling that all she has succeeded in doing is learning more about the model. The epistemological question also has immediate practical relevance, because, increasingly, computational models inform policy decisions. 'Running the numbers' means 'asking a computer' what is likely to happen in the (virtual) world of a model, and acting in the (real) world as a result. This happens all the time, in all manner of ways from the banal (using weather forecasts to decide what to wear at the weekend), to the significant (using economic models to adjust interest rates), to the epoch-making (using climate change models to inform policy on carbon emissions; using war games to help decide whether or not to invade Iraq). It is unfortunate then that we are so badly equipped philosophically and practically to use models of complex systems critically and responsibly.

Oreskes *et al*. (1994) crisply describe the problem: it is impossible to verify the representational truth of any model of an open system. There is a many to one relationship between the structure of models and the behaviour they produce, so that many models can account for the same observed outcome. This is the equifinality problem. One common (incorrect) response to the problem is to examine the internal consistency of the model, and to assume that internal consistency guarantees a true representation of reality. Alternatively, we resort to calibration, and adjust model parameters until a best fit to historical data is achieved. Calibration is a complex technical procedure, but ultimately offers no escape. At best, observational data can be replicated more or less closely, but this provides no guarantee as to a model's accuracy as a representation, nor does it exclude any other model, or another choice of parameters. The common practice of running calibrated models forward in time, beyond the range of observational data, to generate scenarios for decision support is also problematic, since competing models that all produce reasonable fits to historical data can produce widely divergent predictions of future outcomes.

While Oreskes *et al*.'s (1994) conclusions on the impossibility of validation created a stir (Sterman *et al*. 1994), they appear irrefutable. The mainstream response to the critique is the pragmatic claim that a valid model is one that is useful, and that validation criteria can only be determined in light of the purpose of a model (Rykiel 1996). This perspective is similar to the argument for practical adequacy in critical realism: 'To be practically adequate, knowledge must generate expectations about the world and about the results of our actions which are actually realized' (Sayer 1992, 69).

In some contexts, over some spatio-temporal scales, this kind of 'truth' may be achievable, and may even be verifiable. In many domains to which models are applied, it is hard to see how even this level of certainty can be achieved, given the role of models in determining human actions. There is no way of knowing, even with hindsight, that decisions which have been taken based on knowledge derived from a model were the 'right' decisions to make, because the decisions alter the context in which they were made.

Computationally based complexity science thus runs aground on some very old rocks. That said, new strands of thought are beginning to address these difficult questions. Sterman proclaims that 'all models are wrong'[12] (Sterman 2002), and that acknowledging this is vital to progress in using them. In geography, Demeritt (2001), in the context of scientific and policy debates surrounding global warming, unpicks the context of global climate modelling, and suggests that only social and political change can resolve the difficulties. Technical innovations run the risk of obscuring the need for, and thus forestalling, institutional innovation (Shackley *et al.* 1996). Indeed, the mainstream pragmatic response, that validation does not establish the truth of a model, and that agreement on conventions for model evaluation and use is necessary, simply confirms the importance of social conventions in model evaluation. Evans (1997) provides insight into how *unimportant* technical procedures really are when models of a complex entity are routinely consulted. He paints a picture of groups with competing models of the British macro-economy striving to ensure that their predictions do not stray far from the 'gut-feel' consensus of a community of experts. This is a problem if the aim is to produce 'good science'. In the applied policy domain, this is not the purpose, and it may even be comforting to realize that decision processes remain social and political, and not narrowly technocratic, whatever the accompanying rhetoric about prediction.

Conclusion: simulations as geographical narratives?

These philosophical twists and turns bring us full circle. We start from a complex real world phenomenon that defies representation and analysis in traditional mathematical forms, as a result of its constitution by large numbers of interacting entities. We resort to representation of the phenomenon by a computational model, which itself consists of large numbers of interacting software objects representative of real-world entities. The entities and relationships represented are theoretical constructs concerning the subject matter, so that the computational model represents a theory about the world, rather than the world itself. The end result is a model that is itself complex, whose behaviour may be almost as intractably difficult to account for as the world it represents. Connecting the model back to the world it represents is difficult for a number of reasons, principally the equifinality problem, which makes it impossible to judge the relative merits of alternative models on purely technical grounds.

Whatever changes occur in the institutional, political and social context of computational models, the question of how to learn from models remains. It is clear that assessment of the accuracy of a model as a representation must rest on argument about how competing theories are represented in its workings, with calibration and fitting procedures acting as a check on reasoning. So, while we must surely question the adequacy of a model that is incapable of generating results resembling observational data, we can only make broad comparisons between competing models that each provide 'reasonable' fits to observations. Furthermore, critical argument and engagement with underlying theories about the processes represented in models is essential: no purely technical procedure can do better than this.

This suggests a view of complexity science in which models are seen as a source of geographical narratives (stories, plots, dramas) describing how the world is, and how it might be. From this perspective it is vital that modelling is not left to modellers! Instead theories represented in models must be examined and evaluated on their own terms, in the same way that theories are critically judged and evaluated in the social sciences more broadly. It may also be useful to think of complex geographical models as extensions of thought experiments, where the necessary and contingent implications of theories can be examined. Further, admitting that 'all models are wrong' is akin to the realization in post-structural social science that multiple competing accounts of the same settings are possible, and that faced with a diversity of accounts the context and intent of each must be an important element in the evaluation process.

This is unfortunately not how most work is currently evaluated. Frequently, the presentation of a model is overwhelmingly technical. For all the progress that has been made, searing criticism that 'it seems to be assumed that the adoption of mathematical techniques of analysis is all that is required' (Sayer 1976, 250) remains as relevant as ever. This is why Massey's (1999) response to Raper and Livingstone's (1995) work is so germane. Her engagement with deeper representational issues behind a model is welcome and absolutely necessary. There is much to be learned in probing models in this way, because the representation of geographical entities, and of space and time, holds centre stage. Which theories are represented and how, as well as which are not and why not, are questions at the heart of complexity science, which are now also at the heart of quantitative geography in its various forms, whether this is recognized or not.

A modest example is provided by a computational model of gentrification in a London neighbourhood (O'Sullivan 2002). Although the presentation is formal, it is clear that developing this model was an exercise in exploring a particular geographical theory of gentrification – the rent gap hypothesis (Smith 1979). I encountered difficulties in representing the rent gap in an unambiguous way, and this lends support to others who have challenged rent gap theory (see, for example, Ley 1986). Representational difficulties in themselves cannot refute a theory about the world, but they do raise questions about its coherence, or more likely about the possibility of representing a theory adequately with a limited combination of geographical objects. In this case, the exclusion of important actors in Smith's theory – realtors, banks and property developers, for example – fatally compromises the model as a representation of the theory. Even so, the modelling exercise also makes it clear that it is not so much the 'gaps' that give the theory its name that are important, as the actors who perceive the gaps, thereby creating them, and acting on them.

It is problematic that academic and publishing conventions do not enable a full account to be provided of the lessons learned from the modelling process. For journal publication, a model description, in its final form as a representation of (say) rent gap theory is presented, with no place for dead-ends *en route* to that final form, dead-ends that might help to clarify thinking about the theories being examined. It is imperative that ways of opening complex models up to wider scrutiny and criticism are developed if the potential they provide for improved understanding of the geography they represent is to be realized. The process of model development, the possible outcomes it reveals and interpretations of those outcomes, taken together, constitute a geographical narrative, so that modellers become 'makers' of stories. If this perspective is to develop further, so that the presentation of simulation models becomes as routine in human geography as in physical geography, model makers must start telling more interesting stories, so that a wider audience is willing and able to listen constructively. I can only conclude by agreeing with the suggestion of the contributions that inspired this article, that we all 'keep talking'.

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Notes

1 The appropriateness of Aristotle's thoughts on drama was suggested by Stephen Gabel's opening remarks at the *Agent 2003* conference, held at the University of Chicago, 2–4 October.

2 I say 'surprisingly' as a researcher who has built geographical simulation models, and is familiar with this literature. Although questions about how computer models represent geography are familiar in Geographic Information Science (GISci), questioning is usually restricted to practical issues of how a given representation can be most effectively implemented. The classic example is the oft-cited 'raster–vector' debate, which on closer examination is not much of a debate at all (Couclelis 1992). More far-reaching criticism of representation in GISci (Pickles 1995) has mostly fallen on deaf ears, perhaps because even partial acknowledgment of such criticism leaves many of us with nowhere much to go.

3 A secondary context is provided by exchanges in *Geoforum* on the future of geography (Clifford 2002; Johnston 2002; Thrift 2002; Turner 2002). A concern for constructive exchanges across sub-disciplines in geography is noteworthy.

4 A formidable 34 books on complexity and business are reviewed in a special issue of *Emergence* (Lissack *et al.* 1999). The overriding theme is the need to cope with change. This theme leaks into the wider populist business literature, reaching its apotheosis in the disturbingly successful *Who Moved My Cheese?* (Johnson and Blanchard 1998). Thomas Frank (2000) points out the crude functionality of this literature in an age of neo-liberal economics.

5 This overview focuses on dominant North American accounts. Two European schools have contributed to urban modelling. Work by Ilya Prigogine and others (see Prigogine and Stengers 1984 for an accessible account) is antecedent to models developed by Peter Allen and others (see Allen 1997; Pumain *et al.* 1984).

Work by a group in Tel-Aviv (Portugali 2000; Portugali and Benenson 1995; Portugali *et al.* 1994 1997) draws on *synergetics* (Haken and Portugali 1995; Haken 1985 1988). More rarely credited is another complexity concept – from thinking in biology on the self-sustaining nature of organisms – known as *autopoiesis* (see Varela 1981).

6 A view compatible with Tobler's 'first law of geography' that 'everything is related to everything else, but near things are more related than distant things' (Tobler 1970, 236).

7 Many economic models are one-dimensional. Arguably, economics on the circumference of a circle represents only limited progress on the widely discredited economics on the head of a pin!

8 Lock-in has had considerable influence on 'new economy' thinking, justifying companies giving away products to create *de facto* standards (web browsers and mobile phones are the obvious examples). Again, Liebowitz (2002) presents contrary evidence.

9 Massey makes the same point, quoting a personal communication from Martin: 'what economists have failed to recognize is that the notion of "path-dependence" that they now emphasize is itself place-dependent' (1999, 273).

10 The rigidities of economic theory help account for excitable 'paradigm-shift' claims for complexity in economics (see Arthur *et al.* 1997; Anderson *et al.* 1987).

11 Sayer's prescient comment about better models running beyond our ability to handle them is echoed by Helen Couclelis: 'all the simplifying assumptions [. . .] could be relaxed in principle: in practice of course, the result would be forbiddingly complex' (1985, 588).

12 The fuller version of this phrase, originated by George Box, is 'All models are wrong but some are useful' (1979, 202). Arguably, all models are wrong but all are useful to someone (even if the utility is simply to muddy the policy waters . . .).

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